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ABSTRACT

This paper examines the impact of vouchers on student test scores in Dayton, Ohio, New York, New York, and Washington, DC, highlighting New York City parental assessment of private and public schools to investigate why vouchers seem to have differential effects depending on the students' ethnic background. Researchers collected baseline test scores and family data prior to the voucher lotteries, administered the lotteries, and collected follow-up information 1 and 2 years later. At pretest and posttest, students entering grades 1-8 took the Iowa Test of Basic Skills in reading and mathematics. Parents completed surveys regarding satisfaction with their children's schools, involvement in students' education, and demographics. Results varied systematically by ethnicity. There were no significant differences between the test scores of non-black students who moved to private schools and of the control-group students. Vouchers made a substantial, positive difference for black students but did not significantly impact Hispanic American students' test scores. All control group parents were satisfied with their children's education. Hispanic and black parents were very satisfied with private school placements, though they differed in perceptions. Private school impact on black students was not due to school size, class size, school disruptions, school communications, desegregation, dress rules, hallway monitoring, school resources, homework, parent involvement, or student involvement with the school. (Contains 10 tables and 21 endnotes.) (SM)

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EXPLORING EXPLANATIONS FOR ETHNIC DIFFERENCES IN VOUCHER IMPACTS ON STUDENT TEST SCORES

by

Paul E. Peterson
Director, Program on Education Policy and Governance, Harvard University
Senior Fellow, Hoover Institution, Stanford University

and

William G. Howell
Assistant Professor
University of Wisconsin-Madison

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P. Peterson
Harvard Univ., PEPG

Just ten years ago, the only data available about school vouchers came from an experimental public-choice program conducted during the 1960s in Alum Rock, California.[1] But the early and mid-1990s brought new privately and publicly funded voucher programs to cities such as Milwaukee; Dayton, Ohio; Cleveland; Indianapolis; San Antonio; Washington, D.C., and New York City.[2] With them has come a wealth of new research opportunities.

Three were particularly advantageous. The privately funded voucher programs in Dayton, New York City, and the District of Columbia awarded vouchers by lottery, thus creating ideal conditions for a randomized field trial. Prior to conducting the lotteries, our evaluation team collected data on student test scores and family background characteristics. One and two years later, we retested the students. Since the abilities and family backgrounds of the test and control groups were, on average, similar before they entered the lottery, subsequent differences observed between the lottery winners and losers may be attributed to the effects of receiving a voucher. Because of their design, our evaluations of the Dayton, New York City, and Washington, D.C. voucher programs have yielded the best available information on students' test-score outcomes and parental assessments of public and private schools.

Elsewhere we have reported the impact of vouchers on student test-score performance in all three cities, finding positive effects of vouchers on African-American test scores but no effects on the test scores of students from other ethnic backgrounds.[3] In this paper we summarize these findings, then examine parental responses in New York City in order to see whether we can explain why vouchers seem to have differential effects depending on the students' ethnic background.

Prior Research

Our study is not the first to find that private-school effects are concentrated among African American students. The first indication comes from the High School and Beyond data collected by the Department of Education in 1980 and 1982. By surveying and testing a national sample of public and private schools in two waves, the Department of Education generated data on the determinants of gains in high school between a student's sophomore and senior years. In a 1985 issue of *Sociology of Education*, three particularly trenchant essays analyze and interpret these data.[4]

The authors of these essays noted serious disagreements about aggregate private-school impacts. Thomas Hoffer, Andrew Greeley and James Coleman found substantial, positive private-school effects on student test performance, while Douglas Wilms found trivial effects, if any. Christopher Jencks mediates the conflict, reaching judicious conclusions somewhere in the middle.

Few observers, however, noticed in the *Sociology of Education* disputation the discussion of the effects of private schools on minority students. Hoffer, Greeley and Coleman found especially strong positive effects on low-income, minority students. Catholic schooling increased test scores by 4.4 answers, as compared to an impact of 1.6 answers for students generally.[5] Jencks shows that Wilms's data, despite its exclusion of dropouts, also contain positive (though not statistically significant) effects of attending a Catholic school on African Americans' reading scores. Taking all of the evidence from both studies into account, Jencks concludes that "the evidence that Catholic schools are especially helpful for initially disadvantaged students is quite suggestive, though not conclusive."[6] While overall impacts remain contested, those on minorities appear more robust.

Subsequent studies tend to reaffirm Hoffer, Greeley, and Coleman's findings. In an analysis of the National Longitudinal Survey of Youth, Derek Neal finds that students who attend Catholic schools are more likely to graduate from high school and college. The effects, Neal notes, are the greatest among urban minorities. Catholic schools also have a significant, positive effect on black earnings potential, but not whites'.[7] In separate studies, David Figlio and Joseph Stone and William Evans and Robert Schwab generate consistent findings for African Americans.[8] After reviewing the literature concerning the effects of schools on minorities, University of Wisconsin Professor John Witte concludes that studies "indicate a substantial private school advantage in terms of completing high school and enrolling in college, both very important events in predicting future income and well-being. Moreover, . . . the effects were most pronounced for students with achievement test scores in the bottom half of the distribution."[9]

Because they draw upon national datasets, all of these studies are of particular interest. One cannot rule out the possibility, however, that the observed positive effects were due to selection bias, a problem that arises when a population differentiates itself by freely choosing the treatment condition, in this case, attending a private school. This problem may be quite serious if those families with children in private schools looked very different from those with children remaining in public schools. Most of these studies adjusted for observable family background characteristics, such as mother's education, family income, and other demographic factors. Yet one cannot be sure that the adjustments adequately account for an intangible factor—the willingness of a family to pay for their child's tuition, and all that this implies about the importance they place on education. Others performed two-stage regression models to reduce potential selection bias. But it is not easy

to find instrumental variables that are correlated with the type of school students attended but unrelated to the error term in the second-stage equation, as the disagreements between Neal and Figlio and Stone attest to.

The best solution to the self-selection problem is the random assignment of students to test and control groups. Until recently, most evaluations of voucher programs have not utilized a random-assignment research design and therefore have not overcome the possible selection problems. Privately funded programs in Indianapolis, San Antonio, and Milwaukee admitted students on a first-come, first-served basis. And in the state-funded program in Cleveland, though scholarship winners were initially selected by means of a lottery, eventually all applicants were offered a scholarship, thereby precluding the conduct of a randomized experiment. The public Milwaukee program did award vouchers by a lottery, but data collection was incomplete.[10]

As a consequence, the findings presented here on New York, Dayton, and D.C. provide a unique opportunity to examine the effects of school vouchers on students from low-income families who live in central cities. In contrast to prior studies, the evaluation team conducted the lotteries. Follow-up test-score information was obtained from about one-half to two-thirds of the students who participated in the lottery, and baseline data provided information that allowed the analysts to adjust for non-response.

The Programs

In several key respects, the three voucher programs followed similar designs. All were privately funded; all were targeted at students from low-income families, most of whom lived in the inner-city; all provided only partial vouchers that the families were

expected to supplement; and all of the students in the evaluations of these three programs previously had been attending public schools.

New York City. The School Choice Scholarships Foundation (SCSF) in New York City offered 1,300 scholarships worth up to $1,400 annually toward tuition at a private school for at least three years. To qualify for a scholarship, children had to be entering grades 1 through 4, live in New York City, attend a public school at the time of application, and come from families with incomes low enough to qualify for the U.S. government's free school-lunch program. More than 20,000 students applied between February and late April 1997. By the end of the scholarship program's second year, 64 percent of the lottery-winning students were attending a private school.

Dayton, Ohio. In the spring of 1998, Parents Advancing Choice in Education (PACE) offered low-income students in grades K–12 the opportunity to win a scholarship to attend private school. For the 1998–99 school year, PACE offered scholarships to 515 students who were in public schools and to 250 who were already enrolled in private schools in the Dayton metropolitan area. During the program's first year, the PACE scholarships covered 50 percent of tuition at a private school, up to $1,200. Support was guaranteed for at least four years, with a possibility of continuing through high school, provided funds remained available. Of those students offered scholarships, ** percent enrolled in a private school during the second year of the program.

Washington, D.C. The Washington Scholarship Fund (WSF), originally established in 1993, is the oldest of the three programs. By the fall of 1997, the WSF was serving approximately 460 children at 72 private schools. After receiving a large infusion of new

funds from two philanthropists, however, the WSF announced a major expansion in October 1997.

To qualify, applicants had to reside in Washington, D.C., and be entering grades K–8 in the fall of 1998. Families with incomes at or below the poverty line received vouchers that equaled 60 percent of tuition, or $1,700, whichever was less. Families with income above the poverty line received smaller scholarships. Families with incomes higher than two-and-a-half times the poverty line were ineligible. The WSF claims that it will maintain tuition support for at least three years and, if funds remain available, until students complete high school. In April 1998, the WSF awarded more than 1,000 scholarships by lottery, with the majority going to students previously attending a public school. Of those students offered scholarships, 35 percent were still using them to attend a private school in the second year of the program.

Evaluation Procedures

The evaluation procedures used in all three studies conformed to those in randomized field trials. Our evaluation team collected baseline test score and family background information prior to the lottery, administered the lottery, and collected follow-up information one and two years later.

Students took the Iowa Test of Basic Skills (ITBS) in reading and mathematics. Students who were entering grades 1–4 in New York City and grades 2–8 in Dayton (and other parts of Montgomery County, Ohio) and Washington, D.C., were included in the evaluations. Parents filled out surveys on their satisfaction with their children's schools, their involvement in their children's education, and their demographic

characteristics. Students in grades 4 and higher completed similar surveys. In all three cities, the follow-up procedures replicated the pre-lottery procedures: students again took the ITBS in reading and math; parents and older students filled out surveys about their backgrounds and educational experiences.

More than 5,000 students participated in pre-lottery testing in New York City. Of the families did not win the lottery, approximately 1,000 were selected at random to comprise a control group of approximately 960 families. All of these students were attending public schools at the time. In Dayton, 1,440 students were tested pre-lottery, 803 of whom were attending public schools at the time. In Washington, D.C., 2,023 students were tested pre-lottery, of whom 1,582 were attending a public school. Separate lotteries were held in Dayton and D.C. for students who were enrolled in public and private schools; because only public-school children were eligible to apply for a scholarship in New York, there was no need to hold separate public and private lotteries there. In all three cities, only those students who were in public schools at the time of the lottery are included in this study.

In New York City, 42 percent of the students participating in the second year of the evaluation were African-Americans; in Dayton, 47 percent; and in D.C., 94 percent. Hispanic students accounted for 51 percent of the New York City population, 2 percent of Dayton's, and 4 percent of D.C.'s. Whites accounted for 5 percent of New York City's evaluation group, versus 24 percent in Dayton, and 1 percent in D.C. The remaining students came from a variety of other ethnic backgrounds.

In New York City, 80 percent of the students included in the evaluation attended the first-year testing sessions; 66 percent attended the second-year sessions. In D.C., the

response rate after one year was 63 percent; after two years, it was 50 percent. In Dayton, 57 percent of families attended follow-up sessions after one year, and 49 percent after two years.

We are reasonably confident that these modest response rates do not undermine the integrity of our findings. First, with the exception of the second year in New York, response rates were similar for treatment and control groups after one and two years in all three cities. Second, comparisons of baseline test scores and background characteristics revealed only minor differences between the second-year respondents and nonrespondents in all three cities. Finally, to account for the minor differences between respondents and nonrespondents that we did observe, the test scores of children who, based on their demographic characteristics, were more likely to attend follow-up sessions were weighted less heavily, while the test scores of children who were less likely to attend follow-up sessions, but nevertheless did, were weighted more heavily. Given the slight differences between respondents and nonrespondents, however, the weights had little effect on the results.

The randomized lottery ensured that lottery winners as a group were not significantly different from the control group (those who did not win a scholarship). In all three cities, the demographic characteristics and pre-lottery test scores of scholarship winners and losers (the treatment and control groups, respectively) were identical to one another. Only in Dayton were there minor differences in the pre-lottery test scores: those offered a voucher scored 6.5 percentile points lower in math and 3.1 points lower in reading than those not offered a scholarship, a statistically significant difference.

To measure the impact on children's test scores of switching to a private school, we estimate a statistical model that takes into account whether a child attended a public or private school, as well as baseline reading and math test scores. Baseline test scores were included to adjust for the minor baseline differences between the treatment and control groups on the achievement tests and to increase the precision of the estimated impact.

The lottery generated two groups: those who were offered a voucher and those who were not. We're not interested, however, in the effect of being *offered* a voucher. Rather, we're interested in the effect of *using* a voucher to attend a private school. A simple comparison between public and private school students, however, is inappropriate because certain students may be more likely to take advantage of a voucher. Their parents might place greater value on education and be more willing to supplement the voucher, or they may live in a neighborhood with a broader selection of private schools. If these children differ from students who won a voucher but failed to use it in ways that are related to student achievement, it could bias our findings. To solve this problem, we used as an instrumental variable whether or not individual was offered a voucher to predict the probability that she attended a private school; with these predicted values, we then estimated the actual impact of switching from a public to a private school. This two-stage regression technique was first used in medical research and is now commonplace in econometric studies.

Test Score Results

Our findings varied systematically according to the student's ethnicity. In all three cities, there were no significant differences between the test-score performance of

non-African-American students who switched from a public to a private school and the performance of students in the control group – either after one or two years (Table 1).[11] For African-American students, however, vouchers made a substantial difference. In the three cities combined, African American students who switched from public to private schools scored, after one year, 3.3 percentile points higher on the combined math and reading tests (expressed as National Percentile Ranking (NPR) points, which run from 0 to 100 with a national median of 50). After two years, African-American students who used a voucher to enroll in a private school scored 6.3 percentile points higher than their public school peers.

Table 1 also shows that the largest voucher impacts for African-American students were observed in the Washington, D.C. program. African American students who attended D.C. private schools for two years scored 9.0 percentile points higher than students in the control group. The smallest differences after two years were observed in New York City, African- American students attending private schools scored 4.3 percentile points higher than the control group. In Dayton, the difference between test and control groups was 6.5 percentile points.

The average impact of vouchers on the test scores of African-Americans was moderately large. After one year, black students who switched to private schools scored .17 standard deviations higher than the students in the control group. After two years, the difference grew to .33 standard deviations, roughly one-third of the test-score gap between blacks and whites.

The magnitude of the effects can be further assessed by comparing them to the effects observed in an evaluation of a class-size reduction intervention conducted in

Tennessee, the only other major education reform to be subjected to evaluation by means of a randomized field trial. The effects on African Americans of attendance at a private school shown here are larger than the estimated effect of a 7-student reduction in class size. According to a recent reanalysis of data from Tennessee, the class-size reduction effect for African Americans after two years was, on average, 4.9 percentile points, somewhat less than the 6.7 percentile effect of switching to a private school.[12]

It is also of interest to compare the size of the effects of the voucher intervention with the size of the effects reported in the RAND study entitled *Improving School Achievement* released in August 2000.[13] Identifying the most successful states, Texas and North Carolina, which have introduced rigorous accountability systems that involve state-wide testing, the study finds what it says are "remarkable" one-year gains [in math scores] in these states of "as much as 0.06 to 0.07 standard deviation[s] per year"—or 0.12 to 0.14 over two years. The two-year effects of the school voucher intervention on black students observed here are over twice as large.

Controlling for Demographics

Most research on the impact of private schools attempts to control for differences in family income and other background characteristics among students attending public and private schools. When a lottery is used to separate research subjects into experimental and control groups, however, such statistical adjustments are generally unnecessary, given that the two groups being compared are virtually identical to one another.

Nonetheless, a number of analysts have objected to the apparent absence of controls for family background characteristics. Bruce Fuller and his colleagues at the

University of California, Berkeley, for instance, argued that "the experimental group may have been biased as some of the most disadvantaged voucher winners did not switch to a private school, and therefore were excluded from the group (possibly boosting mean achievement levels artificially)." An interest group, People for the American Way, lodged a similar complaint: "The . . . study's key finding improperly compares two dramatically different groups and may well reflect private-school screening-out of the most at-risk students."

In the three cities, roughly half the students took the voucher that was offered to them (the takers) and about half did not (the decliners). However, we did not drop the decliners from the analysis. All voucher applicants were invited to follow-up testing sessions, and each of the families who participated is included in the analysis. To estimate the impact of switching from a public to a private school, we did not simply compare takers with the control group (the decliners and those who didn't win a voucher), as Fuller and his colleagues have contended. Instead, as previously noted, we used the fact that the vouchers were awarded randomly to generate an instrumental variable that generates an unbiased estimate of the effect of switching to a private school.

Even if one were to control for family background characteristics, the results are unlikely to vary significantly. The use of a randomized lottery ensures that the background characteristics of lottery winners and losers will differ significantly. To show this, we recalculated the impact of attending a private school on test scores, this time including explicit controls for the mother's educational level, her employment status, family size, and whether or not the family received welfare. And, as expected, the difference in the combined reading and math test scores of African Americans after two

years in all three cities remain exactly the same – 6.3 NPR points, a statistically significant impact.

The Sore Loser Hypothesis

Since releasing our study, some critics have argued that we have falsely attributed the observed gains in the treatment group to the positive impact of a voucher. Is it not possible that members of the control group are sufficiently frustrated by the experience of applying for a voucher, and then being refused one, that they no longer remain engaged in their child's education? *New York Times* columnist Richard Rothstein, for example, iterates a hypothesis first made by Stanford University Professor Martin Carnoy:

> Parents know if their children got vouchers and this knowledge can affect results. For example, volunteers for vouchers, already more dissatisfied with public schools than others, may have their hopes raised, then dashed when they were not selected for a voucher. Sorely disappointed, they may then demand less of their children in public school.[14]

We know that parents with children in private schools are much more satisfied with their child's school that public-school parents. Is it possible that the test score results that we have observed are due not so much to higher quality schooling in the private sector as to a deterioration in parental involvement in the control group?

To explore this hypothesis, we examined the control group's satisfaction levels at baseline, after one year, and after two years. On each of these occasions, parents were asked: "How satisfied are you with the following aspects of your child's current school?" Items included teaching, school safety, parental involvement, class size, school facility, student respect for teachers, communication regarding student progress, freedom to observe religious traditions, and the school's location.[15] Parents then were given four options, "very satisfied," "satisfied," "dissatisfied," and "very dissatisfied." The results

presented here come from New York City; in future papers we shall report findings for Dayton and D.C.

In all cases, the levels of satisfaction expressed by members of the control group *increased* one year after having been denied a scholarship (table 2). At the end of two years, satisfaction levels did deteriorate somewhat. On five of the nine items, members of the control group reported being slightly less satisfaction about their public schools than they were at baseline. But even after two years the control-group parents remained as satisfied with their public schools as they had been prior to the lottery.

Parental responses to questions about their relationships with their children cast further doubt on the hypothesis that the frustration associated with losing the voucher lottery lead control-group parents to care less about the education of their children. Parents were asked how often they helped their child with homework, talked with their child about school, attended school activities, and worked on school projects. In every case, the answers given by parents with children in the public-school control group after both one and two years remained roughly constant, and closely resembled the responses of parents in the treatment group (Table 3).[16]

These data lend little support for the claim that control-group parents were sore losers. Given that parents knew they had only about a one in twenty chance of winning the lottery, their initial expectations were probably not unduly high. It is hard to imagine, then, that whatever disappointment parents felt when they lost the lottery led to their children's systematic underachievement when tested one and two years later.

Hawthorne Effects[4]

As a corollary to the sore loser hypothesis, Carnoy suggested that our findings might represent Hawthorne effects. If so, then the observed gains for African Americans may have little to do with vouchers per se, but rather the surge of enthusiasm associated with winning a lottery. Upon learning that their children could now attend a private school, the interest and involvement of treatment-group parents in their children's education may have been reinvigorated. A year later, such enthusiasm might wane and children could lose the family support they need to do well in school.

To ascertain whether Hawthorne effects explain the pattern of results, we revisited our measures of parental satisfaction, again focusing on New York City. We constructed an overall index of satisfaction based upon parental responses to all of the parental satisfaction questions, each of which is scored from 1 to 4 depending on the level of satisfaction. The index was then rescaled to have a standard deviation of 1.0. By scaling the measure in this way, one can easily ascertain the "effect sizes" of attending a private school as compared to remaining in public school.

When comparing the impact of attending a private school on parental satisfaction in years one and two, we find some support for the Hawthorne hypothesis. The effect size of attending a private school on the parental satisfaction of African Americans in year one was fully 1.2 standard deviations (see table 4). The effect size attenuates somewhat in year two, dropping to 1.0 standard deviation. Similarly, the size of the effect of attending a private school on the overall grade African American parents gave a school declines from 1.14 standard deviations in year one to 0.86 in year two. What is more, the impact on African American test scores in New York City decreases slightly

from year one to year two, suggesting that Hawthorne effects may be responsible for some of the initial test-score gains.[17]

Other facts, however, cast doubt on Carnoy's intuition. First, the impact of attending a private school on parental satisfaction was quite large in both years one and two. Even after two years, the effect size for parental satisfaction hovers around a full standard deviation. The 0.2 diminution of satisfaction among African American parents that occurred between year one and year two was rather modest when compared to the striking differences in satisfaction with private and public schools that remained after two years.

Second, the impacts on satisfaction rates of Latino and African American parents were comparable in year one, and by year two the impact for Latinos was in fact slightly higher. If all it takes to elevate test scores is to enhance parental satisfaction with a school, then why haven't Latinos posted significant test score gains?

Third, while the trajectory of voucher impacts on test scores in New York City between years one and two declines slightly, in Dayton and D.C. it rises noticeably. When considered together, the overall test score gains observed in all three evaluations—New York, Washington, D. C., and Dayton, Ohio—climb from approximately 3 points in year one to 6 points in year two, a pattern that does not square with the Hawthorne effect hypothesis.

Finally, while they may inform parental satisfaction rates, Hawthorne effects are less likely to drive student achievement. Students participating in voucher experiments are being asked to change schools, form new friendships, adjust to new rules and expectations, and acquire new study habits. In Washington D.C., for instance, older

students who transferred to private schools indicated intense resentment with these changes, which in turn was reflected in their first-year test scores.[18] It is possible, then, that rather than reflecting Hawthorne effects, observed impacts will only increase as students have a greater amount of time to adjust to their new schools, and the educational expectations laid upon them.

Thus far, the preponderance of the evidence suggests that test score gains experienced by African Americans are probably not due to Hawthorne effects. In the future, however, we should be in a stronger position to evaluate the merits of Carnoy's claim. If the gains observed thus far trace back to Hawthorne effects, then the impacts for African Americans observed thus far should subsequently attenuate. If the effects are real, though, the impact of attending a private school on test scores should increase. Data for the third-year evaluation have been collected and will be reported during 2001.

Explaining Ethnic Differences in Voucher Impacts

Neither the absence of background controls, nor the disappointment of losing a voucher, nor Hawthorne effects appear to explain away the observed gains for African Americans, at least after two years. A basic puzzle, therefore, arises. Why should vouchers have a positive impact on the test scores of African American students, but not anybody else? This finding is particularly curious in New York, where African American students posted positive and significant test score gains, but Latinos did not. As poor, minority residents of inner cities, both groups presumably face a common set of educational obstacles. One would think, then, that an intervention that successfully improves the test scores of one group would have a similar impact on the other.

We are best equipped to explore this question in New York City, where a fairly large number of both African America and Latino students participated in the evaluation. In Dayton, and especially in D.C., we simply do not have enough non-African American students to support a sustained analysis of the causal factors that affect the test score performances of different ethnic groups.

The remainder of this paper draws upon parental survey data to assess a broad array of possible explanations for the observed differential race effects: language, school disruptions, class size, school size, parental communications, and a "kitchen sink" models that simultaneously control for a multiplicity of factors. We first test the impact of vouchers for African Americans and Latinos on each of these aspects of a student's education; using these results, we then select a subset of factors to include in the original test score models to see whether or not they reduce the positive effect of vouchers experienced by African Americans. Unfortunately, they do not, neither singularly nor additively. In the end, we are better able to rule out possible explanations than draw positive conclusions.

Language Needs

The fact that African Americans appear to benefit from vouchers, but Latinos do not, may have nothing to do with race per se, and everything to do with language. Private schools may be poorly equipped to deal with students who do not speak English as their primary language; public schools, meanwhile, often have well-established ESL programs and specially trained personnel to deal with the particular cultural and linguistic needs of minority populations. It is possible, then, that the gains associated with a private

education may be transferred only to those students who can function in all-English classrooms.

To test this hypothesis, we compared the impact of switching to a private school on the test scores of Latino students whose primary language (according to their parents) was English with those for whom English was a secondary language. As can be seen in Table 5, the results, if anything, run directly contrary to expectation. Non-English speaking Latinos post slightly positive impacts, while Latinos for whom English is the primary language post slightly negative effects. Neither the positive nor negative impacts, nor the slightly larger differences in impacts, are statistically significant.

These findings do not provide much of a basis on which to judge the ways in which public and private schools deal with students with language needs. They do rule out language, however, as an explanation for why African Americans appear to benefit from vouchers, while Latinos do not. The next section, therefore, shifts focus and examines the characteristics of public and private schools that African Americans and Latinos attended.

Parental Perceptions of Public and Private Schools

Parents accompanied children to the follow-up testing sessions. Because the testing period took over an hour, parents had time to complete fairly lengthy questionnaires about the schools their children were attending. In previous papers, we reported the results from these surveys for all parents regardless of their ethnic background.[19] These results provide information concerning the impact of switching to a private school on parental perceptions of numerous aspects of school life. Generally speaking, in New York, Dayton, and D.C., we found that:

- Private schools have stricter dress codes.
- Hallways in public schools are more closely monitored—students are more likely to need passes to leave the classroom and visitors are more likely to have to get permission slips.
- School disruptions—fighting, cheating, property disruption, student misbehavior, truancy, tardiness, and so forth—are more extensive in public schools.
- Suspension rates are similar in the two sectors.
- Public schools have more physical resources—cafeteria, nurse's office, gymnasium and so forth.
- Public schools have a greater variety of academic programs—special education, advanced education, bilingual education, and so forth.
- Private schools communicate more with parents by means of teacher-parent conferences, parental participation in school, and so forth.
- Students in private school do more homework.
- Private schools have smaller classes
- Private schools have fewer students.
- Although results differ from city to city, on the whole the degree of segregation is similar in the two sectors.
- Parents in both sectors are equally involved in their child's education.
- Parents in both sectors volunteer and participate equally in their child's school.

It is possible that the observed impacts of vouchers on some of these school characteristics vary for different ethnic groups, and therefore may represent likely candidates for why African Americans appear to benefit from choice, while Latinos do not. To explore this possibility, we estimated the impact of attending a private school in New York City separately affected African Americans and Latinos. Some aspects of school life—class size, school size, amount of time spent on homework, degree of ethnic segregation, and suspension rates—could be easily measured by using responses parents gave to a single question. When possible, though, we constructed indices from multiple survey questions that measured the same school characteristic. Questions used to generate each index are reported in the Appendix.

We estimated the impact of switching to a private school on each aspect of school life in the same way that we estimated the impacts on test scores, except that now we do

not control for baseline test scores. The results of this investigation are reported in Tables 6; for parental involvement items, see table 3.

For African Americans, the impacts of switching to a private school on parental perceptions of most aspects of school life were fairly stable from the first to the second year. In both years, African American parents in private schools reported significantly fewer school disruptions (fighting, cheating, property destruction, and so forth) than parents in public schools. They also reported significantly more demanding dress codes, significantly less hallway monitoring, fewer school resources, greater parental communication by the school, more homework, smaller schools, and slightly less involvement in the school (though this difference is statistically significant only in year two). African American parents in private schools, as compared to those in the public-school control group, also reported no differences in suspension rates, in their involvement with their child's education, in the likelihood that their child attended a segregated school, and the number of specific programs (bilingual education special education, advanced education, and so forth) at their child's school. In only one respect did the impacts reported by African American parents differ in years one and two. In year one, African Americans in private schools reported that their child was in a significantly smaller class, but not in year two.

In some respects, the results for Latino parents reveal similar trends. Latino parents in private schools report stricter dress codes, more communication with their schools, more homework, and smaller schools. They also report no difference between the two sectors in suspension rates, the range and variety of school programs, and their own involvement with their child's education.

In several other respects, however, school vouchers appeared to have very different effects on the educations of Latino students. Latino parents who attended private schools, for instance, do not report a reduction in the number of school disruptions. Nor do they report smaller classes or fewer school resources than the control group in either year. Only in one year do Latino parents in private schools report less hallway monitoring. And in one year they report less segregation in the private sector.

The last column of tables 3 and 6 identifies whether or not the observed impacts for African Americans and Latinos are statistically significantly different from one another. Those items with stars represent plausible components of an explanation for why African Americans appear to benefit from vouchers, but Latinos do not. Four aspects of school life stand out—school size, parental communications, class size and school disruptions. In both years, vouchers had a smaller impact on the size of the private schools and classrooms attended by Latino students than they did on those attended by African Americans. Also, while vouchers had a large and positive impact on the communication levels of African Americans, they had relatively small impact on those of Latinos. And perhaps most strikingly, the magnitude of the impacts of attending private school on school disruptions varied dramatically for Latinos and African Americans. Given the sizes and signs of these differences in impacts, these four factors become prime suspects for explaining the differential race effects of vouchers on test scores.

Other differences are evident in one of the two years. In year two the impact of attending a private school on hallway monitoring was significantly smaller for Latinos than it was for African Americans. The year-two impact on school resources, meanwhile,

was larger. Given the sign of these differences, however, they probably do not explain why African American students voucher students log the only test score gains. Could it be that African American students in private schools benefit from the fact that their hallways are less closely monitored? Do black students in private schools benefit because they have fewer school resources – cafeteria, gymnasium, nurse's office, and the like? Probably not.

Note that from these impacts we cannot infer whether African Americans are coming from a particularly poor lot of public schools, or are gaining access to a particularly effective group of private schools. All that we know is that along some dimensions, the impact of the switch for African Americans was greater than that for Latinos. With our short list of four factors that may explain the observed differential race effects, however, we can now proceed to the second step in this analysis – showing the relationship between these factors and student achievement.

Do School Size, Class Size, School Disruptions, and Communication Levels Explain Why Vouchers in New York City Lift African Americans' Test Scores, but Latinos'?

To check, we regressed class size, school disruptions, school size, and school communications on student test scores after both the first and second year. Also included in the equation was an indicator variable for whether or not the student had been offered a voucher, as well as baseline test scores and lottery indicators. If these four school characteristics explain the differential impact of a voucher offer on the two ethnic groups, the impact of the voucher on African Americans should diminish or entirely disappear once they are added to the model.

The results for African Americans are reported in Table 7. The first column reports the impact on African American test scores of being offered a voucher in New York City after one year: 4.6 percentile points. In column three, we report the same effect after two years, 3.3 percentile points.[1]

In columns two and four, we report the effects of a voucher offer in years one and two, respectively, controlling for parental reports on school disruptions, school communications, class size, and school size. If these factors explain the impact of a voucher on African American test scores, then the effect of a voucher should diminish or disappear. Unfortunately, this does not happen. Instead, the size of the impact remains essentially constant. These four factors, neither separately nor combined, do not explain why African Americans perform better on tests when given an opportunity to attend a private school. Parenthetically, only one of the four items in either year—the school disruption index in year two—has a significant and direct impact on African Americans' test scores. The others do not appear to have any causal impact at all.

Table 8 provides a similar set of equations for Latino students. The offer of a voucher has no impact on student performance either before or after these items are included in the equation. In addition, school disruptions appear to have a negative direct impact on student performance, especially in year one. Class size also has an effect, but its sign is perverse: Latinos do better in larger classes. This correlation could be caused,

[1] (Please note that these equations estimate the effect of a voucher offer, not that of actually switching to a private school, as reported in Table 1. The latter estimates are larger because not everyone offered a voucher makes use of one, and some families who are not offered a voucher attend a private school anyway. For two reasons, this section focuses on the effects of a voucher offer. First, because of the random process by which vouchers were awarded, the estimate of a voucher offer can be recovered with a simple ordinary least squares equation. And second, there is a linear relationship between these two estimates: the impact of switching from a public to a private school uses as a benchmark the impact of a voucher offer, and adjusts the estimate upwards or downwards depending upon the percentage of students in the treatment and control groups that attend private schools.)

however, by the assignment of Latino students with language or learning difficulties to smaller sized classes.

The technically more sophisticated reader may wish to see these same results presented in one equation that includes interaction terms. This information is provided in table 9. The regression includes the overall impact of a voucher offer on both groups of students (which in this equation specifies the impact on Latinos), the interaction between a voucher offer and whether or not the student is an African American (the sum of the estimated coefficients on this interaction term and the main treatment variable is the impact on African Americans), an indicator variable for African Americans, and separate terms for school disruptions, school communications with parents, school size, and class size. Also included in the equation are controls for baseline test scores, other variables necessitated by the particular features of the lottery administered in New York City, and all of their respective interaction terms.

The estimated effects in this saturated model replicate those recovered when running separate models for African Americans and Latinos. As before, these factors do not explain the impact of vouchers on African American test scores; the direct effects of these four variables remain the same as well. Interestingly, though, the indicator variable for African American is negative and highly significant, suggesting that African Americans, on average, scored lower than their Latino peers.

Do All Measured Factors, Taken in Combination, Explain the Impact of Vouchers on African American Test Scores?

Perhaps the factors that impact African American test scores are not the same ones that distinguish the impact of vouchers on the perceptions of African American and

Latino parents. Perhaps it is some other factor or all factors combined that account for the differential race effects that we observe. To see whether or not this is the case, we conducted a kitchen sink analysis, one that included every survey item in a single model, along with all relevant interaction terms. Such a model is not a very good way of estimating the impact of any particular aspect of school life on student test score performance. Because few of the indices measure distinct school characteristics, the estimated impact of each is being partially estimated by others. However, this approach allows us to ascertain whether measurable aspects of school life help explain the private school advantage for African Americans.

Table 10 answers the question in the negative. Even when we include all items—not only school disruptions, school communications, school size, and class size but also suspensions, dress rules, hallway monitoring, school resources, homework, segregation, parental involvement with child, and parents involvement with the school—in the equation, the impact of the voucher offer on African American test scores remains intact.

Discussion

If African Americans learn more in New York City private schools than they do in public schools, and if the private-school impact is not due to school size, class size, school disruptions, school communications, desegregation, dress rules, hallway monitoring, school resources, homework, level of parental involvement with child's education, and their involvement with the school, then what does explain the difference?

Parental perceptions are not always as precise as we might like, and so we need not prematurely rule out these school characteristics as possible explanations for the differential race effects that vouchers seem to generate. Still, though, we remain

impressed how similar are the patterns of parental response from one year to the next and one city to another. If parents were responding to questions more or less randomly, then we should observe different patterns that vary across city and over time. Instead, the pattern of parental responses is remarkably stable.

It is possible, though, that the voucher impacts derive not from these item considered separately or additively, but through some complex interaction among some or all of the variables that our models have not estimated. Perhaps it is the interaction between school disruptions and school size that counts? Or the interaction between parental-school communications and class size? Indeed, different aspects of school life may come together in different ways for African Americans and Latinos, generating very different test scores outcomes for the two groups. In the future, we plan to estimate additional models that account for the interaction between different school factors, rather than just the main effects of each taken one at a time.

It is also possible that private-school impacts are due to instructional factors that none of the items in our parental survey adequately measure. Perhaps the disparities between the quality of teachers for African American students in public and private schools are much wider than those for Latino students. Perhaps African American students are particularly and uniquely receptive to teaching techniques that are more prevalent in private schools. Recent research has shown that teacher effectiveness can have a large impact on student test-score performance.[20] Our models, however, do not include any measures of curriculum, teaching techniques, the expectations that teachers place on their students, or teacher quality. Such factors might be the key to understanding why African Americans benefit from choice, but Latinos do not.

Finally, the effects may have nothing to do with the characteristics of public and private schools that African Americans and Latinos attend. They may instead derive from the quality of the peer groups at these schools. Richard Rothstein thinks positive effects arise when voucher recipients "are surrounded by pupils with higher academic expectations."[21] If African Americans attend private schools with a particularly elite group of classmates, while the peer groups of Latinos who switch from public to private school change very little, then peer effects may lay at the heart of the story we are trying to uncover.

We still do not know what it is about private schools that make them successful, at least for African Americans. And without an answer, it remains unclear how, or even whether, public schools can introduce appropriate reforms that clearly and identifiably benefit African American students. Future pilot studies that contain a larger number of subjects and collect a broader array of information may unearth some of the reasons why at least some students appear to benefit from choice. A good place to begin this research, it seems to us, would be in the District of Columbia, where voucher impacts were large and the population to be served is largely African American.

TABLE 1: Impact of Switching to a Private School on Test Score Performance

	Year 1 (Percentiles)	(N)	Year 2 (Percentiles)	(N)
African Americans				
New York City	5.8**	623	4.3**	497
Dayton, Ohio	3.3	296	6.5*	273
Washington, D.C.	-0.9	891	9.0***	700
Average Impact	3.3		6.3**	
All Other Ethnic Groups				
New York City	-1.4	704	-0.9	612
Dayton, Ohio	1.0	108	-0.2	96
Washington, D.C.	7.4	39	0.1	44
Average Impact	0.2		-0.8	

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Weighted two-stage least squares regressions performed; treatment status used as instrument. Impacts expressed in terms of national percentile rankings. In New York City, 2.8 percent of the African American control group in the year 2 models attended a private school for one of two years; in Dayton, 2.0 percent of the African American control group in the year 2 models attended a private school in the second but not the first year; and in D.C., 3.7 percent of the African American control group in the year 2 models attended a private school in the second year but not the first year. In New York City, all non-African Americans in this study consist of Latinos; in Dayton, the vast majority consists of Caucasians.

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Table 2.—Percentage of Control Group 'Very Satisfied' with their Public School at Baseline and after One and Two Years

How satisfied are you with the following aspects of your child's current school?

% very satisfied with:	**1997: Baseline**	**1998: Year One**	**1999: Year Two**
Teaching:	14	23	10
School Safety:	13	21	9
Parental Involvement:	11	19	12
Class Size:	7	12	7
School Facility:	9	14	5
Student Respect for Teachers:	18	21	11
Communication Regarding Student Progress	18	23	19
Freedom to Observe Religious Traditions	8	9	5
Location	25	34	28

Baseline satisfaction scores are for all families not offered a scholarship from which control group is drawn. Year-one and year-two satisfaction scores are weighted so that generalizations can be made to the baseline applicant pool.

TABLE 3: Impacts of Switching to a Private School on Measures of Parental Involvement of African Americans and Latinos in New York City

	African Americans (Percentiles)		(N)	Latinos (Percentiles)		(N)	Diff. In Impacts
Parental Involvement with Child's Education							
Year 1 Impact	-0.04	[0.11]	532	-0.11	[0.15]	568	--
Year 2 Impact	-0.06	[0.13]	470	-0.15	[0.16]	529	--
Parental Involvement with Child's School							
Year 1 Impact	-0.14	[0.14]	519	0.04	[0.14]	553	--
Year 2 Impact	-0.23*	[0.14]	470	-0.08	[0.15]	529	--

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. The last column denotes whether the difference in the estimated impacts for African Americans and Latinos is statistically significant. Standard errors reported in parentheses. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for lottery indicators. Impacts expressed in terms of effect sizes.

TABLE 4: Impacts of Switching to a Private School on Measures of Satisfaction for African Americans and Latinos in New York City

	African Americans (Percentiles)		(N)	Latinos (Percentiles)		(N)	Diff. In Impacts
Satisfaction Index							
Year 1 Impact	1.23***	[0.12]	533	0.98***	[0.13]	567	**
Year 2 Impact	1.00***	[0.12]	466	1.14***	[0.14]	529	--
Overall Grade							
Year 1 Impact	1.14***	[0.12]	533	0.75***	[0.13]	567	***
Year 2 Impact	0.86***	[0.13]	469	1.01***	[0.14]	530	--

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. The last column denotes whether the difference in the estimated impacts for African Americans and Latinos is statistically significant. Standard errors reported in parentheses. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for lottery indicators. Impacts expressed in terms of effect sizes.

TABLE 5: Impact of Switching to a Private School on Test Score Performance in New York for Latinos who Speak English as a Primary and Secondary Language

	English Primary (Percentiles)		(N)	English Secondary (Percentiles)		(N)
Year 1 Impact	-2.56	[2.54]	399	2.30	[2.91]	305
Year 2 Impact	-1.55	[3.03]	342	3.14	[3.37]	290

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Standard errors reported in parentheses. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for baseline test scores and lottery indicators. In no year are the estimated impacts for the two groups of Latinos statistically significantly different from one another.

TABLE 6: Impacts of Switching to a Private School on Characteristics of Schools Attended by African Americans and Latinos in New York City

	African Americans (Percentiles)		(N)	Latinos (Percentiles)		(N)	Diff. In Impacts
School Disruptions							
Year 1 Impact	-0.46***	[0.13]	525	-0.02	[0.15]	564	**
Year 2 Impact	-0.28**	[0.13]	465	-0.16	[0.16]	523	--
Suspensions							
Year 1 Impact	0.01	[0.16]	515	-0.13	[0.11]	547	--
Year 2 Impact	0.02	[0.18]	463	0.09	[0.10]	526	--
Dress Rules							
Year 1 Impact	1.47***	[0.10]	526	1.30***	[0.12]	559	**
Year 2 Impact	1.06***	[0.12]	461	0.90***	[0.14]	512	--
Hallway Monitors							
Year 1 Impact	-0.62***	[0.13]	517	-0.55***	[0.14]	556	--
Year 2 Impact	-0.67***	[0.13]	464	-0.10	[0.15]	519	***
School Resources							
Year 1 Impact	-0.25*	[0.14]	530	-0.15	[0.14]	567	--
Year 2 Impact	-0.49***	[0.14]	468	-0.08	[0.14]	526	**
School Programs							
Year 1 Impact	0.22	[0.13]	528	0.05	[0.14]	564	--
Year 2 Impact	-0.16	[0.15]	462	-0.04	[0.15]	522	--
School Communicate w/ Parents							
Year 1 Impact	0.71***	[0.13]	533	0.30**	[0.13]	564	***
Year 2 Impact	0.78***	[0.14]	469	0.43***	[0.15]	528	**
Amount of Homework							
Year 1 Impact	0.64***	[0.13]	528	0.49***	[0.03]	565	--
Year 2 Impact	0.48***	[0.13]	470	0.33**	[0.15]	527	--
Class Size							
Year 1 Impact	-0.61***	[0.13]	516	-0.04	[0.15]	540	***
Year 2 Impact	-0.21	[0.14]	460	0.01	[0.16]	512	--
School Size							
Year 1 Impact	-0.88***	[0.15]	367	-0.47***	[0.17]	364	**
Year 2 Impact	-0.82***	[0.15]	353	-0.54***	[0.15]	408	*
Racial Segregation							
Year 1 Impact	-0.03	[0.13]	518	-0.16	[0.15]	553	--
Year 2 Impact	-0.13	[0.15]	457	-0.24*	[0.14]	513	--

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. The last column denotes whether the difference in the estimated impacts for African Americans and Latinos is statistically significant. Standard errors reported in parentheses. Weighted two-stage least squares regressions performed; treatment status used as instrument. All models control for lottery indicators. Impacts expressed in terms of effect sizes.

Table 7: Impact of a Voucher Offer on African Americans' Test Scores: Likely Suspects

	Year 1		Year 2	
	(1)	(2)	(3)	(4)
Treatment	4.55***	4.61***	3.27**	3.56**
School Disruptions	--	-0.74	--	-5.71***
Communication	--	-1.86	--	-3.33
School Size	--	-2.22	--	1.39
Class Size	--	2.06	--	-0.55
Constant	-3.38	-1.41	0.79	4.18
(N)	624	624	497	497
Adjusted R^2	.53	.52	.43	.43

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Weighted least squares regressions performed. Impacts expressed in terms of national percentile rankings. All models control for baseline test scores and lottery indicators. Means imputed for missing data on covariates drawn from survey.

Table 8: Impact of a Voucher Offer on Latinos' Test Scores: Likely Suspects

	Year 1		Year 2	
	(1)	(2)	(3)	(4)
Treatment	-0.97	-1.13	-0.60	-0.68
School Disruptions	--	-6.47***	--	-2.44
Communication	--	5.65*	--	-1.35
School Size	--	2.80	--	3.25
Class Size	--	6.28*	--	14.25***
Constant	20.04	11.67	11.47	3.27
(N)	709	709	612	612
Adjusted R^2	.44	.46	.45	.47

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Weighted least squares regressions performed. Impacts expressed in terms of national percentile rankings. All models control for baseline test scores and lottery indicators. Means imputed for missing data on covariates drawn from survey.

Table 9: Impact of a Voucher Offer on Test Scores, Single Model with Interactions

	Year 1		Year 2	
	(1)	(2)	(3)	(4)
Treatment	-0.97	-1.13	-0.60	-0.68
Treat*AA	5.53***	5.74***	3.87*	4.24*
African American	-23.41*	-13.08	-10.68	0.92
School Disruptions	--	-6.47***	--	-2.44
Communication	--	5.65*	--	-1.35
School Size	--	2.80	--	3.25
Class Size	--	6.28*	--	14.25***
Disruptions*AA	--	5.72**	--	-3.27
Comm.*AA	--	-7.51*	--	-1.98
School Size*AA	--	-5.02	--	-1.86
Class Size*AA	--	-4.21	--	-14.79***
Constant	21.17	12.04	12.61	5.83
(N)	1333	1333	1109	1109
Adjusted R^2	.48	.49	.45	.46

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Weighted least squares regressions performed. Impacts expressed in terms of national percentile rankings. All models control for baseline test scores, lottery indicators, and their associated interactions. Only Latinos and African Americans are included in the models. Means imputed for missing data on covariates drawn from survey.

Table 10: Impact of a Voucher Offer on Test Scores: The Kitchen Sink

	Year 1	Year 2
Treatment	-1.24	-0.81
Treat*African American	5.74***	4.83**
African American	-32.21**	-1.89
School Disruptions	-5.86***	-2.20
Communication	5.72*	0.56
School Size	2.84	5.28*
Class Size	6.27*	13.94***
Suspensions	-13.49***	-5.52
Dress Rules	-2.10	-1.83
Hallway Monitors	-0.93	-4.96*
School Resources	-4.67	-5.81
Homework	6.79**	7.82**
Segregation	1.37	0.56
Involve w/ Child	-8.96***	0.06
Involve w/ School	-0.78	-5.92*
Disruptions *AA	5.21**	-3.42
Comm.*AA	-10.21**	-4.16
School Size*AA	-6.54*	-3.49
Class Size*AA	-4.68	-13.67**
Suspensions*AA	12.75**	7.43
Dress Rules*AA	3.63	-0.92
Hallway*AA	1.31	6.85*
Resources*AA	14.22***	2.18
Homework*AA	-7.02*	-2.21
Segregation*AA	-0.97	-1.16
Inv. w/ Child*AA	10.02**	-4.88
Inv. w/ School*AA	1.50	7.21
Constant	22.71	10.94
(N)	1333	1109
Adjusted R^2	.50	.47

* significant at .10 level, 2-tailed test; ** .05 level; *** .01 level. Weighted least squares regressions performed. Impacts expressed in terms of national percentile rankings. All models control for baseline test scores, lottery indicators, and their associated interactions. Only Latinos and African Americans are included in the models. Means imputed for missing data on covariates drawn from survey.

APPENDIX

Depending upon which year the surveys were administered, indices were constructed from all, or a subset, of the items that follow. Response categories are available upon request.

School Disruptions: "How serious are the following problems at this child's school? Very serious, somewhat serious, or not serious?" Kids destroying property; kids being late for schools; kids missing classes; fighting; cheating; racial conflict; racial conflict; guns or other weapons; drugs or alcohol.
Suspensions: "During this past year, was this child ever suspended for disciplinary reasons?"
Dress Rules: "Are students required to wear a uniform?" "Are certain forms of dress forbidden?"
Hallway Monitors: "Are visitors required to sign in at main office?" "Are hall passes required to leave class?"
School Resources: "At the school this child attends, which of the following programs or facilities are available to students?" A computer lab; a library; a gym; a cafeteria; child counselors; a nurses' office.
School Programs: "At the school this child attends, which of the following programs or facilities are available to students?" Special programs for non-English speakers; individual tutors; special programs for students with learning problems; special programs for advanced learners; a music program; an arts program; an after-school program.
School Communication with Parents: "Do the following practices exist in this child's school?" Parents informed about student grades halfway through the grading period; parents notified when student sent to the office the first time for disruptive behavior; parents speak to classes about their jobs; parents participate in instruction; parent open-house or back-to-school night held at school; regular parent/teacher conferences held; parents receive notes about this student from this child's teacher's; parents receive a newsletter about what's going on in this child's school/classroom.
Amount of Homework: "Approximately how much homework is assigned on an average day?"
Class Size: "Approximately how many students are in this child's class?"
School Size: "Approximately how large is the school this child attends?"
Racial Segregation: "What proportion of students in this child's classroom is minority?" Percent responding "everyone" or "90-100 percent."
Parental Involvement with Child's Education: "In the past month, how often did you do the following?" Help this child with his or her homework; help this child with reading or math that was not part of his or her homework; talk with this child about his or her experiences at school; attend school activities; work on school projects.
Parental Involvement with Child's School: "How many parent-teacher conferences did you attend this school year?" "How many hours have you volunteered in this child's school this past month?" "Are you a member of a PTA or other similar organization (Parent's Council, for example)?"
Satisfaction Index: "How satisfied are you with the following aspects of this child's current school?" Location of school; school safety; teaching; how much school involves parents; class sizes; school facilities; student respect of teachers; how much teachers inform parents of students' progress; how much students can observe religious traditions; parental support for the school; discipline; clarity of school goals; teamwork among staff; teaching values; academic quality; the sports program; what is taught in the school.
Overall Grade: "What overall grade would you give this child's current school?"

Endnotes

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[2] Disparate findings have emerged from these studies. For example, one analysis of the Milwaukee choice experiment found test score gains in reading and math, particularly after students had been enrolled for three or more years, while another study found gains only in math, and a third found gains in neither subject. Jay P. Greene, Paul E. Peterson, and Jiangtao Du, "School Choice in Milwaukee: A Randomized Experiment," in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Washington, D. C.: Brookings, 1998), pp. 335-56; Cecilia Rouse, "Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program," Department of Economics, Princeton University, 1997; John F. Witte, "Achievement Effects of the Milwaukee Voucher Program," paper presented at the 1997 annual meeting of the American Economics Association. On the Cleveland program, see Jay P. Greene, William G. Howell, and Paul E. Peterson, "Lessons from the Cleveland Scholarship Program," in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Washington, D. C.: Brookings, 1998), pp. 357-92; Kim K. Metcalf, William J. Boone, Frances K. Stage, Todd L. Chilton, Patty Muller, and Polly Tait, "A Comparative Evaluation of the Cleveland Scholarship and Tutoring Grant Program: Year One: 1996-97," School of

Education, Smith Research Center, Indiana University, March 1998. Greene, Peterson, and Du, 1998 report results from analyses of experimental data; the other studies are based upon analyses of non-experimental data.

[3] William G. Howell, Patrick J. Wolf, Paul E. Peterson, and David E. Campbell, "Test-Score Effects of School Vouchers in Dayton, Ohio, New York City, and Washington, D. C.: Evidence from Randomized Field Trials," Paper prepared for the annual meetings of the American Political Science Association, Washington, D. C., September 2000. Available at www.ksg.harvard.edu/pepg/.

Other papers that report results from the evaluations include the following: William G. Howell and Paul E. Peterson, "School Choice in Dayton, Ohio: An Evaluation After One Year," Paper prepared for the Conference on Charters, Vouchers and Public Education, 2000, (Program on Education Policy and Governance, Kennedy School of Government, Harvard University, Cambridge). Website address: www.ksg.harvard.edu/pepg/; Patrick J. Wolf, William G. Howell and Paul E. Peterson, " School Choice in Washington, DC: An Evaluation after One Year," (Paper prepared for the Conference on Charters, Vouchers and Public Education, 2000, sponsored by the Program on Education Policy and Governance, Kennedy School of Government, Harvard University, Cambridge, MA; Website address: www.ksg.harvare.edu/pepg/. First-year results from the New York City evaluation are reported in Paul E. Peterson, David E. Myers, William G. Howell, and Daniel P. Mayer, "The Effects of School Choice in New York City," in Susan B. Mayer and Paul E. Peterson, eds., *Earning and Learning: How Schools Matter* (Washington, D.C.: Brookings, 1999), Ch. 12. For results from the second-year evaluation of New York City's voucher program, see David Myers, Paul E. Peterson, Daniel Mayer, Julia Chou, and William P. Howell, "School Choice in New York City after Two Years: An Evaluation of the School Choice Scholarships Program," September 2000.

Occasional Paper, Program on Education Policy and Governance, Taubman Center on State and Local Government, Kennedy School of Government, Harvard University). Report available at www.ksg.harvard.edu/pepg/.

[4] Thomas Hoffer, Andrew Greeley, and James Coleman, "Achievement Growth in Public and Catholic Schools" *Sociology of Education* (April, 1985) 58:74-97; Douglas Wilms, "Catholic School Effects on Academic Achievement: New Evidence from the High School and Beyond Follow-up Study," *Sociology of Education* (April, 1985) 58:98-114; Christopher Jencks, "How Much Do High School Students Learn?" *Sociology of Education* April, 1985 5:8: 128-135.

[5] Hoffer, Greeley and Coleman, Table 1.7, 1.8., pp. 80-81; these are the estimates of effects when controlling for background characteristics and years in Catholic school.

[6] Jencks, p. 134.

[7] Derek Neal, "The Effects of Catholic Secondary Schooling on Educational Achievement," *Journal of Labor Economics* (1997) 15:1,pt.1, pp. 98-123.

[8] William N. Evans and Robert M. Schwab, "Who Benefits from Private Education? Evidence from Quantile Regressions," (Department of Economics, University of Maryland, 1993); David Figlio and Joe Stone, "School Choice and Student Performance: Are Private Schools Really Better?" (University of Wisconsin Institute for Research on Poverty, 1977).

[9] John F. Witte, "School Choice and Student Performance," in Helen F. Ladd, ed., *Holding Schools Accountable: Performance-Based Reform in Education* (Washington, D. C.: Brookings, 1996), p. 167. Other studies finding positive educational benefits from attending private schools include James S. Coleman, Thomas Hoffer, and Sally Kilgore, *High School Achievement* (New York: Basic Books, 1982); John E. Chubb and Terry M. Moe, *Politics, Markets, and America's Schools* (Washington: Brookings 1990); Critiques of these studies have been prepared by Arthur S. Goldberger and Glen G. Cain, "The Causal Analysis of Cognitive Outcomes in the Coleman,

Hoffer, and Kilgore Report," *Sociology of Education*, vol. 55 (April-July 1982), pp. 103-22.

[10] Results from these evaluations are reported in Paul E. Peterson and Bryan C. Hassel, eds., *Learning from School Choice* (Brookings, 1998).

[11] In this paper we shall focus on overall or combined test score results, which simply represent the average of the math and reading components. When we use one-hour testing sessions to gauge student performance, their combined reading and math scores serve as a better indicator of student achievement than either subcomponent separately. Theoretically, the more test items used, the more likely we are to measure performance accurately. As Jencks (p.131) points out with respect to Wilms's findings, "some of the apparent 'noise' in Wilms's Table 7 might have disappeared had he collapsed different sophomore tests into a single composite."

[12] Krueger, p. 525.

[13] Also, see Ann Flanagan, Jennifer Kawata and Stephanie Williamson. 2000. *Improving Student Achievement: What NAEP Test Scores Tell Us* (Santa Monica, CA: RAND Corporation, 2000), p. 59.

[14] Richard Rothstein, "Judging Vouchers' Merits Proves to be Difficult Task," *New York Times*, December 13, 2000, p. A25. Martin Carnoy, American Prospect, January 2001.,

[15] Items listed in previous section include only those that formed part of the baseline survey. Later surveys added additional items.

[16] Peterson, Myers, and Howell, 1998, Table 13.

[17] Such has been the interpretation given by some to a similar pattern of results observed in the Tennessee class size study, another randomized experiment in which participants know whether they are in the test or control group. In that study almost all gains from class size reduction take place in the first year, with very few gains in subsequent years. Caroline Hoxby, Economics Journal.

[18] Patrick J. Wolf, William G. Howell and Paul E. Peterson, " School Choice in Washington, DC: An Evaluation after One Year," (Paper prepared for the Conference on Charters, Vouchers and Public Education, 2000, sponsored by the Program on Education Policy and Governance, Kennedy School of Government, Harvard University, Cambridge, MA; Website address: www.ksg.harvare.edu/pepg/

[19] See studies cited in note 3.

[20] Steven G. Rivkin and Eric A. Hanushek and John F. Kain, "Teachers, Schools and Academic Achievement, April, 2000. (Manuscript.)

[21] Rothstein, p. A25.